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ABSTRACT

This paper criticizes the Coleman Report for not measuring how much extra learning took place as a result of favorable changes in school inputs. Other limitations were that the range of school inputs were confined to those present in the schools surveyed and that no indication was given as to whether the educational investment was worthwhile. It is also argued that educational change should be aimed at eliminating poverty and that lifetime income gain resulting from learning gain should be greater than costs of producing that change. Lifetime income gain criterion, it is held, can be measured in four steps: (1) using standardized tests to measure how much extra learning takes place as a result of a given educational change; (2) calculating the yearly equivalent learning gain; (3) calculating direct income return to the individual who attends school one year longer by comparing lifetime income histories of individuals with different years of schooling; and (4) calculating estimated income gain derived from learning change by multiplying percentage of a year's worth of learning by lifetime income return associated with an extra year of education. (KG)

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EDUCATION AND POVERTY REVISITED

By Thomas I. Ribich
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For five years or so I've held a belief -- born out of research efforts -- that new educational spending was not a good way to attack the problem of poverty in the United States. Progressively, it has become less and less interesting to hold to this attitude. For a while I could amaze my friends and alarm men of responsibility when I told them about my research findings. Then the Coleman report came out, and most of those who saw it came around to a similar point of view. It wasn't long before boiled-down descriptions of the Coleman report began to appear in a wide variety of scholarly and semi-scholarly sources. Then the news began to spread that some of the compensatory education programs were not working out as well as initially reported. Soon the same was being said about Head Start, and negative research reports on Head Start followed. As a last straw, President Nixon recently submitted an education report to Congress -- and the press -- that contends that our recent educational programs, especially those aimed at poor children, have not been getting very far. In all, my views about the anti-poverty ineffectiveness of increased educational spending are now hard to distinguish from current conventional wisdom.

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Being part of the conventional wisdom is uncomfortable for at least two reasons. First, when your views are close to accepted doctrine people are usually less intrigued by what you have to say on the matter -- they have the impression they read all about it in a recent Sunday supplement, if not before. Second, there is the worry that the conventional

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wisdom is always wrong; that any point of view that is widely accepted must be incorrect because it misses the finer nuances of the situation that can turn the argument on its head, or incorrect because the popular consolidation of a view point, especially on social questions, ususally brings forth effective refutation from people on the frontiers of research and practice, and a new point of view begins to emerge, at least among insiders. Maybe my research is already passe and irrelevant, less than two years after it appeared in print.

After some reflection, and a dash of additional research, largely spurred by the pressure to present a paper to the distinguished group present here today, I convinced myself that these worries should not concern me. My brand of pessimism about education is not really the same as the one in current vogue and reasons for it are not the same either. And the new evidence that has appeared has not yet driven me fron my general point of view.

Let me quickly add that I stand ready to be convinced otherwise, but the sort of evidence that I have seen so far doesn't yet do the job for me, though there are some recent findings that have brightened my view a little.

It is on the matter of evidence that I would like to spend much of my time this afternoon. There are a series of questions about evidence I would like to pose -- and try to answer. The questions I have in mind are the following:

1) What kind of evidence is appropriate for judging the wisdom of investing in a given educational change?

2) What does the appropriate kind of evidence suggest about the the general power of education to alleviate poverty.

3) What sorts of biases exist in that evidence?

4) Are there serious contradictions in the available evidence?

5) What is the explanation for the conclusion suggested by that evidence?

6) What should be the public policy response to these findings?

Let me try to answer those questions in the order presented, and then, after that, I would like to make a few remarks on some recent proposals for educational reform.

First <u>what kind of evidence is appropriate for judging the wisdom of investing in a particular educational change</u>? There are a number of terms in that question which deserve to be defined. What I mean by most of the terms will, I hope, become apparent in the course of my discussion, but there is one term I would like to clear up at the outset -- and that is the term investing. Investing usually means an outlay of money or real resources in the hopes of a reasonable rate of return, or something better than a reasonable rate. That is generally the sense in which I wish to use it here. Some educational changes, of course, do not involve a special new outlay of money or resources -- they just mean doing different things with the same amount of money. Those kinds of changes are <u>relatively</u> easy to evaluate -- if one approach seems to result in more learning than another, which costs the same amount, that can be taken as excellent prima facie evidence that this particular approach is preferable. The problem is more difficult when the comparison is between educational approaches that cost different amounts. As it turns out, most educational comparisons involve programs that <u>do</u> cost different amounts, and most educational improvements usually do require an extra outlay of

money. It is these sorts of educational changes and improvements which I want to concentrate on for the time being, not only because such changes are more typical, but also because it is the contemplation about these sorts of changes which will, I think, shed the most light on the larger question of whether education should receive special emphasis when attacking poverty.

So, to return to the question what sort of evidence is needed to judge whether a particular educational change is worthwhile or not, let me first try to answer that question by an example, an example of the type of evidence that doesn't make the grade, that is not sufficient. The example I have in mind is the Coleman Report.[1]

There are a number of quarrels that can be picked with that document, but many such quarrels can also be picked with just about any cross-section analysis which uses statistical controls rather than experimentally controlled observations. Beyond that, however, is the problem that the particular statistical measures that are used in the Coleman report fail to be appropriate for judging whether an investment in a particular type of educational change does or does not yield a reasonable rate of return.

The Coleman report concentrates on verbal ability test scores as its measure of output, or return, from educational inputs. Now the emphasis on verbal ability doesn't seem like such a bad selection, given that this measure is pretty highly correlated with most other measures of learning that one might consider. But how do we judge the difference between a satisfactory and an unsatisfactory change in verbal ability test scores as a result of a change in some particular school input? As we do cross-section comparison among schools, suppose we find that one

school with much better qualified teachers and much better facilities than some other school -- all other factors being equivalent -- induces students to score 5-points better on a verbal ability test than does the school with less abundant resources. How can we judge whether that was a reasonable return for the better teachers and the better facilities? What is a five-point gain worth?

The Coleman Report does not answer this question, and it doesn't even ask it. The main reason it does not ask this question is that the Report does not concern itself with the size of the test score gains associated with various educational inputs and other factors, it does not report on how much extra learning takes place with a favorable change in school inputs. Instead, the report concentrates on the percentage of variance (or scatter) in the test scores of pupils that is statistically explained by the various factors. It measures how much things like home environment, socio-economic status of classmates, and school inputs contribute to the explaining the differences in test scores found among pupils. In statistical jargon, the Coleman Report measures the contribution to R^2 and does not concern itself with the regression coefficients.

There is a convenience in that statistical measure. It allows comparisons among the major factors contributing to pupil achievement, without having to worry about the units of measurement. Measuring the size of the test score gain associated with a favorable change in one of the factors -- which is what a regression coefficient does -- gives rise to an ambiguity that requires some extra work to clear things up and make the independent variables comparable. What meaning can be attached to statements such as "pupils seem to gain one point on the verbal ability

test with an addition of 300 volumes to the school library, and they score one point better if their father is a high school graduate rather than a high school dropout." Since number of volumes and father's education are not measured in the same units there is no direct way to evaluate which of the two contributing factors is most important -- and it is the goal of ranking contributing factors in terms of importance which absorbs the attention of the Coleman Report. Measuring the contribution to variance explained is a shortcut way to accomplish such a ranking without worrying about comparability of units.

Unfortunately, contribution to the amount of variance that can be explained does not tell us if a given educational change is worthwhile undertaking or not. Alterations in a given school input -- whether it be the quality of teachers or the availability of science lab facilities -- may not explain much of the total variance but it may still produce a respectable gain in learning when compared to the costs of the investment needed to produce that alteration. From the Coleman Report we cannot tell whether or not this is the case. The right measures are just not present.

It would seem interesting to note, however, that the Coleman Report finds that differences in school inputs seem to add only modestly to the total explanation of why pupils perform differently, and that home background factors and quality of an individual's classmates seem to explain much more. The relatively large role played by family background would seem to suggest we are not able to produce equivalent average performance among children with different socio-economic backgrounds simply by changing school inputs. So that even if one doesn't know the exact, or even the approximate, pay off rate of a given educational investment, one might still conclude

from the evidence in the Coleman Report that education cannot do the whole job of making low status individuals perform as well in school as high status children. If one is indeed interested in, and hopeful of, producing equality of performances for the present generation of pupils, the Coleman report would seem to provide a convincing council of despair.

But even that seemingly direct conclusion cannot be read from the Coleman report. The range of variation of school inputs in the Coleman report was limited by what was present in the schools being surveyed. Though the range of variation in inputs doesn't explain much of the total variation in verbal test scores, there is nothing in the Coleman Report which indicates that we could not produce as much learning, and verbal ability proficiency, for low-status children as for high status ones if we simply undertook a very intensive program for the low status children -- if we did something like triple or quadruple the resource inputs going into the education of low status children. The only question is whether it is worth it or not, whether the effort would be prohibitively expensive, or whether there might be a preferable approach that will move us toward equality and poverty elimination with greater efficiency than other alternatives available.

In short, the Coleman Report does not contribute a great deal to the question of whether, and how much, special emphasis should be placed on education. It does not give much indication of whether any educational investment is worthwhile or not, be it a large investment or a small one. Without information of some kind of rates of payoff -- in some sort of appropriate units of account -- little can be said about how much we should rely on education trying to do something about poverty.[2]

The example of the Coleman Report suggests that the following pieces of information are needed:

(1) a measure of how much more is learned with a given educational change

(2) an estimate of what that particular change is worth in some sense or other, and

(3) the costs of that educational change.

The information for the first and third needs are fairly easy to come by. How much more is learned can be estimated by looking at the actual test score gains that result with educational changes. Costs can be estimated by the right kind of digging into school budgets. The question of how much the change in learning was worth is, on the other hand, a quite difficult and controversial matter.

Let me propose one standard that in many cases might be quite sufficient, and is at the very least a good starting point. The standard I would put forward is the lifetime gain in income experienced by those who experience the gain in learning. If we are concerned foremost about poverty, and we think of poverty in its most common ordinary language meaning -- having a very low level of material well-being -- then the prediction of how much more a person will earn because of a given gain in learning would seem to be the paramount consideration.

If we do calculate the income gain implications of a learning gain, the problem still remains of deciding what kind of gain in income is a satisfactory gain. That judgement cannot be made independent of the costs of the particular educational alteration being considered. If a given educational change financed from public funds yields a lifetime gain in income of a few thousand dollars to a handful of people, but the educational

change costs millions of dollars, it is a doubtful proposition that the educational change was worthwhile -- at least not in terms of doing something about poverty. If those millions of dollars were given outright to those who are poor (say through a negative income tax) or if it is devoted to any of a number of other expenditures (like housing or medical care) that improve directly the material well-being of the poor, then surely a great deal more poverty could be alleviated spending the funds in this way rather than going ahead with the education program.

The standard, then, that I would suggest is that a learning gain associated with a given educational change aimed at doing something about poverty cannot be clearly justified unless the income gain that results from it is something greater than the costs of producing that change. If it is not greater than the costs, then more poverty can be alleviated by spending the same amount of money directly on raising the well-being of the poor.

Now there are a number of objections that might be raised against such a criterion. Perhaps the most serious criticism boils down to the argument that there's a lot more reasons why we might want to improve the education of our young people besides the one of increasing their economic productivity and income; so that even if the income gain enjoyed by those experiencing the educational improvement falls somewhat short of costs of the improvement, it may still be worthwhile, on balance, to undertake. While that is generally a reasonable argument, it is still not unreasonable to insist that improving the material well-being comes first when we are talking about those families in the community who have very low incomes.

It is quite easy to be diverted into furthering other less pressing goals once one begins to worry about the other dimensions of any program that starts out as means for raising the economic well-being of the poor. Urban renewal and the community action programs are cases in point.

It is also well to remember that programs which directly improve the material circumstances of the poor also have additional favorable effects. For instance, a negative income tax which relieves some of the more serious forms of material deprivation also can work to improve the general outlook of the individuals affected, can reduce anti-social behavior, and can have the effect of helping along considerably the school performance of the children who are in the families being helped. The favorable side effects of dollars spent directly on alleviating poverty may easily be more valuable than the social and personal worth of educational improvements other than the value of raising income.

The empirical question of determining just how much these side effects are worth is exceedingly difficult. No researcher has got very far on that one at all. If and when the side effects for varions are measured, they can be blended into the analysis. And anyone who has some strong views about which types of programs have the more important indirect effects can try to blend them in himself; but as long as the reduction of poverty continues to be a primary social goal of public policy, the comparison of income gains with costs should figure heavily in decisions.

There is more that can be said about this criterion, but for the moment, I will continue under the assumption that the standard is acceptable and turn to the second question on the list: <u>What does the</u>

appropriate kind of evidence suggest about the general power of education to alleviate poverty?

In light of what was stated just a little earlier that question can be rephrased to say "what is the payoff rate from improving the education of children of low socio-economic status?" What, in other words, is the estimated amount of the lifetime income gain experienced by individuals benefiting from improved education, and how does this income gain compare with costs?

As can be readily imagined the most difficult part of answering that question empirically is the matter of estimating the lifetime income implications of recent educational changes. The proceedure I undertook in my work involves a fairly risky calculation, but it seems to be the only way, at present, to provide at least a rough estimate of the needed information.[3] The approach can be broken down into four steps.

First, observe how much extra learning takes place as a result of a given educational change, this measured by differences or changes in scores on standardized tests--hopefully a comprehensive battery of such tests.

Second, calculate how much is normally learned over a one-year period under normal conditions, and find out what percentage of a years worth of learning took place as a result of the educational change. (In other words, find the "yearly equivalant" learning gain.)

Third, calculate the direct income return to an individual who goes to school one year longer by comparing the lifetime income history of individuals who complete a different number of years of schooling.

Fourth, calculate the estimated income gain that will come from the learning change by multiplying the percentage of a year's worth of learning that took place (the yearly equivalant test score change) times the lifetime income return associated with an extra year of education. Thus, if an individual, as a result of a given learning experience, learns 20% of a year more in the same amount of time, that is treated as if he continued his education 20% of a year longer than he would have otherwise.

After that a number of other fairly technical adjustments must be made, the most quantitatively important being discounting the estimated increase in future lifetime income, to account for time preference. Whenever we must compare dollar amounts in the future with dollar amounts in the present, some kind of discounting of future dollar amounts is necessary because of a rationally motivated preference for present dollars over future ones. Present dollars can earn interest, so that a dollar received or paid now is more valuable than a dollar paid or received in some future period -- the effects of inflation aside. I used a 5 percent discount rate, a pretty standard figure, to do the required discounting.

The basic data on educational changes came from a wide variety of sources -- the result of a scavenger hunt through published research results, government bureaus, educational research organizations, and boards of education across the country. I came up with data on four different types of educational changes -- compensatory education programs, pre-school programs, dropout prevention programs, and the effect of simply spending more money and devoting more resource to public education. The calculations indicate some interesting differences in payoff rates among

these various types of programs, but what was more interesting was the result that payoff rates were generally quite low. The estimated, discounted gains in lifetime income from all varieties of educational change that I examined turned out to be less than the costs of the programs. The typical sort of relationship was for income gains to be around 60 percent of the costs of the educational improvement.[4]

All of this evidence came from programs that were initiated before the start of the big push by the Federal Government to channel more resources into education. This was not by choice but by necessity, since all the data gathering took place at about the same time that the large-scale Federal efforts were just getting under way. No great loss in relevance seemed to result from this, however. The educational changes considered were quite similar to some that were built into the Federal efforts. Research specifically on the large scale federal program that have been performed recently have born this out more or less.

Fairly complete reports were released last summer on both Title I of the 1965 Education Act and Head Start Program.[5] Both show statistically insignificant changes in test scores as a result of these two types of programs. That would suggest Head Start and Title I were doubtful ventures even if they did not use up real resources. A statistically insignificant gain in the test scores suggests a statistically insignificant income gain as well. Moreover, even if one ignores the problem of statistical significance and uses the differences that did appear -- which were slightly in favor of those experiencing the program -- the test score gain implies a gain in income which is not large enough to justify costs.

Other studies tend to add further corroboration to the conclusion that the payoff rate to improved education is quite low. Studies done in New York State and for large-city school systems in a number of metropolitan areas show only small improvements in test scores associated with improvements in school inputs.[6] And recent independent reports of compensatory education programs of various types still seem to be indicating a bad batting average for such programs.

In summary, the body of available evidence generally suggests that we do not know how to add more resources to the educational process in such a way that we succeed in raising income by a greater dollar amount than the cost of the additional resources. This result shows up both in the recent large efforts of the Federal Government and in analysis dealing with programs and changes unconnected with those recent efforts. It isn't simply the dead hand of Washington that is at fault. The implication in that educational improvements, of the ones we have had recent experience with, do not seem to be very good anti-poverty devices.

That brings us to the third question: <u>What sorts of biases exist in the evidence</u>? Several sorts of possible bias exist, but it is not clear which direction much of it might go. For the important biases whose direction <u>can</u> be determined, the thrust is predominantly towards making things look better than they really are.

Let me quickly mention just two of these biases. First of all, there is a fairly serious bias in the sampling that went into nearly all the studies, including my own. Sampling in all cases depended to some degree on the cooperation of local authorities, and there are numerous indications that those schools and school districts that did not cooperate had programs that were, on average, worse than the typical program.

On the other end of the calculating technique, the estimated income gains experienced by those who benefitted from the educational programs cannot be thought of as all contributing to the diminution of poverty. Many of the children who profited from the experience -- even many of those from very poor families -- would have ended up earning non-poverty incomes anyway. So that the estimated income gains cannot all be thought of as contributing to the goal of poverty reduction. Some of the income gains will just be making more affluent some individuals who are destined for affluence anyway. A recent calculation I tried suggests that the dollar amount of future poverty eliminated may amount to only about a third of the total income gain generated from anti-poverty education programs. This suggests that the amount of poverty eliminated for each dollar spent is not 60 cents, as my initial calculations suggested but instead only 20 cents. With this factor taken into account, a dollar spent directly on poverty elimination seems far and away to be more effective than a dollar spent on anti-poverty education.

Fourth question, <u>are there serious contradictions in the available evidence</u>? If one has to give a "yes" or "no" answer to the question, the answer would have to be "no". Rather than inconsistancy and contradiction, there is, as noted earlier, a startling amount of corroboration.

This does not mean there are no exceptions. Programs can be found that <u>do</u> work, that do yield satisfactory learning gains when compared to costs. But I have not yet come across any indications that these results can be replicated. They appear to be more the result of fortuitous circumstances and the heroic efforts of an inspired educational leader

rather than elements that can be closely controlled and reproduced. Many of them seem to flourish because of an exciting experimental atmosphere, a factor which generally loses its effect as the experiment is transplanted on a large scale.

There is one variety of broadly based statistical evidence that does, however, seem to be at odds with this pessimistic finding for education. This is the calculated rate of return for completing more years of schooling. That evidence, produced and elaborated upon by several well-known economists, seems to say that there is a high payoff for the individual from completing an additional number of years of education.[7] There has been considerable controversy about the meaningfulness of that evidence. Many economists hold the view that the calculated payoff rates have severe problems of statistical control that make the high rates more illusion than reality. But it seems to me that the really crucial point is that we have only limited social control over reducing the numbers of individuals who drop out early from the regular educational sequence. We could raise the minimum school leaving age, but it is unlikely that those who were compelled to continue their schooling would learn and earn as much as those who now continue schooling voluntarily. The rate of return calculated upon past experience would then fail to be a meaningful guide. We can, and have, conducted widespread anti-dropout campaigns, but the ones tried in the past indicate a quite small return for the resources invested.[8]

In short, the rate of return information related to school continuance seems to have no readily available policy tool that allows us to take

advantage of that apparently respectable payoff rate, even if that payoff rate is real and not illusory. Such calculations do not seem to offer serious contradictions to the general conclusion.

The fifth question, what is the explanation for the generally low returns from educational investments made in behalf of poor children, would seem to have many possible answers. One possibility is that children from families of low socio-economic status are unusually impervious to educational improvements. That answer doesn't have much appeal or theoretical grounds, and there is no evidence to my knowledge that high status children respond more dramatically than low status children to educational improvements. High status children generally do a lot better in school, but they do not seem to learn a great deal more when subjected to an improved educational setting. I know of no study that clearly shows that increased educational spending on affluent children results in a higher payoff rate than it does for poor children. And there are substantial indications that the payoff rates for both types of children turn out to be about the same.[9]

Another possible explanation is that we are not doing the right things in the schools, they are not organized properly, or we approach children the wrong way, or we are not relevant enough, or we are too permissive -- the list of possible flaws is very long, and some criticism seems to be in direct contradiction with others. And hard evidence is hard to come by on what specific changes in approach really produce consistently better results.

There are, however, a number of more thorough going reorganizations of the educational process which might drastically change the efficiency

with which we use educational resources. But our lack of actual experience with those reorganizations leave it unclear as to whether these large changes will lead to drastically less or drastically more efficiency.

There is another possible explanation, and it is the one that would seem to be most consistent with the evidence and with economic theory. That explanation is that we have simply over-invested in education already. That we have gone about as far as we can go.

It would seem quite possible that adding additional and higher quality resources to the educational process doesn't seem to do much because we have already done a lot. As a nation, we have relied on our educational system to do a great deal. As a country with a long tradition of upward social mobility we have placed more hopes in education, than have most countries, in education as an avenue to social and economic advance. Perhaps we have overdone it. Perhaps we have pushed our overall investment in education so far that -- while an individual still can rise in relative status if he gets a better education -- extra educational investments of nearly any kind do not pay a very high real rate of return.

An additional reason for thinking that we are over-invested, at least in economic terms, is that we have expanded and intensified education for other than economic reasons. We have pushed to a high level of literacy and general education not only to make individuals productive and capable of earning a good living, but also to promote individual fullfillment and the smooth working of social relations. We may not have succeeded very well in achieving those ends, but we have surely tried. That being the case, it would seem quite reasonable that we have pushed education

quite a way past the point of strict economic justification. If, by magic, we took a years worth of learning away from everyone in the economy, it is quite likely that this would do very little genuine economic harm. Additional educational investments may have reached a point of rapidly diminishing returns, and there may be very little we can do about it.

What does all this mean for new courses of action, or to put it in the terms of the sixth and last question, what should be the public policy response to the findings of a low rate of return to additional educational investments? The possible answers are, once again, numerous.

As a starter, it seems reasonable to go along with the strategy outlined by the new Administration in Washington -- rely more heavily on some form of a negative income tax and job training in trying to alleviate poverty. Job training, in sharp contrast to our recent efforts in education, seems to yield a very high rate of economic payoff.[10] And a negative income tax, even in the inhibited form proposed by the administration, seems certain to do a great deal of good. The experiments that are being conducted in New Jersey are quite encouraging as far as administrative work-ability is concerned, and some evidence has turned up from this experiment that a well designed negative income tax can increase, rather than reduce, work incentives.

But what about the education front? The implications of the data seem to suggest that accelerating educational spending under present conditions would be a stark example of throwing good money after bad.

One possibility is to let educational investments expand more slowly than they have been expanding, under the fatalistic belief that we are indeed overinvested in education and there is not much to do about it except to try to let that over-investment situation correct itself gradually and with a minimum of disruption.

A more positive and active alternative is to push forward with large scale experiments involving very substantial changes in the way education is organized and conducted. There is nothing in the studies cited above that clearly rules out the possibility that a voucher system to support private schools or the setting up of kibbutz-like children's centers would not yield a high return. Changes of this sort are high risk undertakings, however, and it would seem a good idea to try at least a few experiments before making another leap in national policy. If more attention had been paid to the evidence that was in existance in the early 1960's (albeit a little hard to uncover), we could have avoided some costly mistakes. Now seems to be the time to get started on experiments that test whether quantum jumps in approach may succeed where small changes failed.

It should be emphasized, though, that large changes in educational programs are more likely to produce statistically significant gains in learning than do small changes that are equally well conceived. Statistical significance does not verify the worthwhileness of making that change. The important thing to observe, as argued throughout this paper, is the rate of payoff, to compare the worth of the learning gains with the costs of the undertaking.

Finally, let me urge that we evaluate new experiments objectively, that we do not let experiments commit us to large scale undertakings because we are embarrassed to admit that they didn't work out well. There is nothing inconsistent in having high hopes but retaining a sense of noncommitment, however unheroic that may sound.

FOOTNOTES

[1] James S. Coleman, et. al., Equality of Educational Opportunity (Washington: U. S. Office of Education, 1966.)

[2] For additional criticism of the Coleman Report see Samuel S. Bowden and Henry M. Levin,"The Determinents of Scholastic Achievement -- An Appraisal of Some Recent Evidence," Journal of Human Resources, 3 (Winter, 1968) and Glenn Cain and Harold W. Watts, Problems in Making Policy Inferences from the Coleman Report. Discussion Paper 28-68, Institute for Research on Poverty, University of Wisconsin, Madison, 1968.

[3] For a more complete description of methodology and results see Thomas I. Ribich, Education and Poverty, Brookings, Washington, 1968.

[4] Ibid. Chaps. 3, 4, and 5.

[5] Harry Piccarello, Evaluation of Title I, in Inequality: Studies in Elementary and Secondary Education,(Office of Education: Washington, 1969.)

[6] Jesse Bankhead, et. al., Input and Output in Large-City School Systems, Syracuse University Press 1967; and Herbert J. Kiesling. Measuring a Local Government Service: A Study of Efficiency of School Districts in New York State, (Ph.D. Dissertation, Harvard, 1965)

[7] See, for instance, W. Lee Hansen, "Total and Private Rates of Return to Schooling", Journal of Political Economy, (April, 1963) pp. 128-40.

[8] Burton Weisbrod, Preventing High School Dropouts in R. Dorfman [ed.] Measuring Benefits of Government Investments, Brookings Institution, 1965, pp. 117-149. Arthur J. Corrazzini,"Prevention of High School Dropouts: An Analysis of Costs and Benefits.

[9] Education and Poverty, op. cit., p. 126.

[10] Ibid., pp. 38-50.